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Published in:
Information science : from the development of the discipline to social interaction

Publication date:
1995

Document version
Publisher's PDF, also known as Version of record

Citation for published version (APA):
Hjørland, B. (1995). Overload, quality and changing conceptual frameworks. In J. Olaisen, E. Munch-Petersen, & P. Wilson (Eds.), *Information science : from the development of the discipline to social interaction* (pp. 35-67). Scandinavian University Press.

64: "The credence which it is rational to give to a statement at a given time must be determined by the degree of confirmation . . which the statement possesses on the total evidence available at the time." Cf. Jon Elster, Nuts and Bolts for the Social Sciences (Cambridge: Cambridge University Press, 1989), p. 109: "These estimates are rational, in the sense of taking acount of all available information . ." Or the economist Kenneth J. Arrow, "Rationality of self and others," in Rational choice: the contrast between economics and psychology, edited by Robin M. Hogarth and Melvin W. Reder (Chicago: University of Chicago Press, 1987), p. 206: "The common understanding [of the term 'rationality'] is . . the complete exploitation of information, sound reasoning, and so forth."

(2) Communications of the ACM v. 35, no. 12, December 1992.

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1996

Overload, Quality and Changing Conceptual Frameworks

Patrick Wilson's important essay earlier in this book describes the consequences of information overload from the perspective of the individual researcher or information user. In this article, I will supplement Wilson's thoughts from the perspective of the sociology and theory of science.

The Nature of Changing Conceptual Frameworks

In addition to pointing out that unmanageably large amounts of information produce overload, Wilson notes that constantly changing conceptual frameworks increase the problem dramatically. What is the background and nature of these constantly shifting conceptual frameworks? We could think of them as important advances in scientific theory, but conceptual frameworks generally shift for other reasons, many of them less flattering.

Wilson's concept of the changing conceptual framework can be translated into a variety of concepts such as "theory," "paradigm," "ideology," "fad" and "fashion." Some of these concepts have a negative connotation and many see some of them as lying outside the boundaries of science. It is not difficult, however, to show that phenomena such as these can be found in scientific literature, especially in the behavioral and social sciences Think, for example, of the wide range of psychotherapies, of which there are over five hundred kinds. The field of management is also known for many theories of questionable nature.

As information scientists, we are interested in general patterns and tendencies. We are not interested in analyzing problematic fields such as psychotherapy or management science in themselves. We are interested in these fields as possibly extreme examples of a more general pattern. Just the fact that such unscientific activities are carried out in the name of science reveals something about problems in the quality control mechanisms of science and scholarship.

The positivist perspective in science is now presumed to be dead, but in practice, it is a hidden and very much alive influence (see Tolman 1992). According to the positivist perspective, concepts such as "paradigms," "ideologies" and "fads" do not existent in science. Science consists of a neutral collection of value-free data. Other theories of science, such as Kuhn's theory (1970), suggest the opposite. There are two issues to consider in contemplating Kuhn's position. The first is that Kuhn's theory is not constructed on the basis of problematic and immature fields such as psychotherapy or management. Kuhn considers what are often–and somewhat misleadingly-called the "hard sciences." Second, this theory shows that important factors are or can be unconscious on the part of researchers. According to Kuhn, there are important trends in science which can only in part be explained by the rational, conscious, publicly formulated principles that constitute the official explanation of science. Scientific disciplines are influenced - to a greater or lesser degree - by unscientific factors such as ideological tendencies. These can be intra-disciplinary or transdisciplinary and they can be shared with non-scientific society.

In this paper, I will try to outline three types of conceptual frameworks or ideologies that cause problems in the quality of scientific literature and overload for the user: basic world view, conceptual frameworks related to scientific function and short-sighted pragmatism.

Frameworks Related to Basic World Views

Perhaps the most fundamental question in psychology and the social sciences is the question of the determinants of human behavior, biological or socio-cultural. This example will be used to say something about an important kind of changing conceptual framework concerning basic attitudes toward world view, human nature, society, science, reality and other issues. Degler (1991) offers a scholarly exposition of shifting tendencies in American social thought regarding the influence of Darwinism. Allen (1993) offers a very valuable discussion and review of Degler's book.

In reading Allen's review, I am confirmed in the impression that there is a psychological tendency to regard one's own world view as scientific, that is, as based on scientific research, while the opposing view is seen as ideological and unscientific. Biologism and socio-cul-

turalism are two viewpoints. Both viewpoints have strong scientific bases. Both have also been influenced by clearly ideological and unscientific perspectives. Most research on the problem in this century has not illuminated the mechanisms by which the biological and the socio-cultural factors interact in the development of human behavior. Instead, most research has been rooted in ideological assumptions which have not been questioned. The result has been theoretical stagnation and an accumulation of trivia. The hegemony of behaviorism in much 20th century psychology is recognized today as a problematic and unfruitful chapter in the history of psychology. This is but one important trend in research which has not seriously questioned its own basic assumptions. The really alarming issue is not that behaviorism flourished but that it so effectively supplanted alternative views that there were few alternatives in the discipline.

Research methods in the human sciences often concentrate on what could be called technical problems. There has been important progress in research methodology which illuminate the importance of biological factors for behavior such as twin studies. It seems clear to me, however, that this kind of technical methodology can never in itself settle the question. It is important to have a broader view. It is necessary to interpret single studies in the light of other studies. It is important for the human sciences to develop a historical perspective on their own research.

The works considered here (Allen, 1993; Degler, 1991) offer a fruitful background for researchers in the field. That is, technical methods must be supplemented by different kinds of historical and philosophical methods. It is important for future researchers to develop an attitude that is critical toward ideology. Researchers must learn to see both opposing viewpoints as influenced by ideas in a larger economic, social and political context. A balanced view allows researchers to minimize the possibility that ideological influence only one of these viewpoints is able to exert a dominant ideological influence. As an ideal, there should be symmetry rather than asymmetry in the perception of ideological tendencies. The best safeguard against onesidedness is reading fundamental literature that presents both views in a broad historical and cultural context. A blind trust in technical methodology increases the probability that a research will come to support one ideological tendency in a more or less unconscious fashion. This conclusion has important consequences for the normative

criteria of information needs. The exclusive study of recent literature offers too narrow a context. It is a kind of ideology to think of recent literature as the best without being able to set it in an historical perspective.

Allen (p. 458) rightly points out that ideology is not always a bad thing. Darwin was ideological inspired by the political and economic views of Ricardo, Malthus and Adam Smith. This did not reduce the validity of his theory. Ideology can be a great creative force in science and it can also blind the researcher. The aim is not to pretend there is no ideology in good science, but to bring the ideology to the surface for examination–to see if it provides insight or obscures our vision.

Allen's criticism of Degler's view is not based on Degler's tendency to favor biology. Biological factors do play an important role in human behavior. The issue is the interaction of biological and social factors and the ways in which we can approach a deeper understanding. Here Allen finds that systems theory can be a help: "All systems -physical, biological and social - function at various hierarchical levels of organization, and that although each level builds on and is related to the levels below, none can be understood only in terms of its lower level components. Each level must be studied on its own terms, with methods appropriate to that level. Human social phenomena may be based on various biological levels of organization below the social: organismic, cellular or molecular. But data from those levels do not map in any direct way onto phenomena at the societal level. The social sciences are indeed based on biology, but they are not reducible to biology. That is the crux of the entire debate . ."

With regard to information overload, we can see that it is easy to be overloaded by the thousands of scientific studies of a narrow, technical nature which do not contribute a broader perspective to the questions at hand. This is related to the cumulative nature of scientific research–or it absence. This, in turn, is related to the theoretical integration of the single findings. If science is not driven by interest in the object of its research, but drifts instead under ideological influences, then theoretical integration is impossible. Overload is not so much a question of the number of documents as of their conclusiveness and perspective, in essence, their quality. It is important that scientific papers are conclusive, either positive or negative. If papers are clear and conclusive in a negative way, it is in principle possible to make information systems that eliminate these studies. Overload is more difficult to cope with in the case of unclear, inconclusive papers if it is not outright impossible. Each scientist must interpret them for himself. Knowledge is not cumulative. Users become overloaded.

Another conclusion is important. According to Karl Popper, scientific theories compete in a marketplace of ideas. The best theories are those which can withstand most criticism. These are the fittest theories, and these are the theories that survive. The problem with Popper's view is that some viewpoints lack the resources to produce the same number and kinds of arguments as more prevalent ideological theories. If strong social forces promote theories related to one ideology while you are trying to balance the viewpoint, you are going to be overloaded. In Contrast, if you have some kind of power, for example, the money to hire people, then these people will be interested in giving you relevant information on their qualifications in an orderly and concentrated way. It will be the applicants for a job who do the hard work of collecting and organizing information, not you. Therefore, overload is also a social condition associated with lack of power and influence. One of the latest mechanisms to cope with overload, the so-called think tanks, must be seen in this perspective. These are organizations in which information is collected, filtered and processed often from a specific point of view, often political (see Ricci, 1993).

Conceptual Frameworks Related to Scientific Functions

Scientists function according to their attitudes toward themselves, toward science and toward the scientific process of accumulating knowledge. Scientists develop their attitudes as members of society. These attitudes are also shaped by the scientific education, by explicit philosophies of science and by peer influence. These attitudes can be based on science or on ideology to a greater or lesser degree, and researchers can be conscious or unconscious of their own attitudes in varying proportion.

In my opinion, clear examples of scientists avoiding difficult and time-consuming activities can be called avoidance behavior. Besides being an interest and a calling, science is also a career. Scientists sometimes face a dilemma between doing what scientific problems demand of them and doing what careers demand. They can face the temptation to make research look more scientific and convincing than

it often is. They are tempted to produce articles with no new important knowledge, an act that leads to the overproduction of scientific literature. They are increasingly forced to sell themselves in the market and to produce results in which the truth-value of research is not as important as its money-value for the researchers. This is but one of the mechanisms that increase overload.

Again, it is important to balance two viewpoints. Scientists can be too safe. They can have too little motivation and too little interaction with the rest of society or they can be too much concerned with factors other than scientific inquiry. The important thing for us as information scientists interested in the problems of information use and overload is to be able to analyze these issues from perspectives rooted in the sociology and theory of science. The concrete analysis will, of course, vary from field to field and from time to time. Here, we are trying to identify possible mechanisms, analyzing how the conditions under which scientists work affecting scientific production and cause information overload for the user.

One issue in scientific malfunction is simple fraud. A growing literature indicates that this has become an increasingly important problem. This is the made clear in books and articles by Braunwald (1987), Broad and Wade (1982), Garfield (1987a, 1987b, 1987c, 1990), Kochan and Budd (1992), Stewart and Feder (1987) and Weinstein (1979). This problem has also had an effect on information science, most visible in practices for handling errata and retractions at National Library of Medicine, Washington (Kotzin and Schuyler, 1989).

Fraudulent literature will naturally cause overload for users. First, they must identify documents in a polluted sea of references. Then they must assess claims concerning fraud and discard fraudulent material. Then they must correct false conclusions drawn on the basis of fraudulent material. They must do all this if they care seriously about the quality of their work and are not simply willing to get by Kochan and Budd (1992) refer to cases of documented fraud in the cardiological literature and demonstrate rather shockingly, that fraudulent results are cited in the medical literature as if they were true. This is the case in spite of the fact that fraudulent references in the MEDLINE database were assigned the descriptor *Retraction of Publications* along with other precautions.

How should we interpret this knowledge concerning fraudulent research? Garfield (1987) seems to think of fraud as isolated cases, the personal defects of a very few individuals. He believes fraud has nothing to do with the conditions under which researchers work or with the culture of science or of the larger society. More modern psychological and sociological theories view the matter in a different way. Persons becoming alcoholics were seen in traditional psychiatric paradigms in the same way that Garfield regards fraud, as something genetic, as a stable, personal defect in the single person. Modern social-psychological theory, however, considers the interaction between the alcoholic personality and the rest of society. It is clear that the more that alcohol is consumed in a population, the greater the incidence of alcohol-related diseases. Alcoholism, criminality and related effects are not merely stable individual errors in the mental mechanisms of selected individuals. They represent an interaction between several kinds of weak or predisposed personalities and some larger cultural tendencies. It is therefore possible to learn something about a society by observing its deviant personalities. As mentioned earlier, I am not as interested in special cases of scientific malfunction as in possible explanations of a more general nature.

I will not consider fraudulent research as a problem in itself but as an indicator of stress in the system. After all, demonstrated cases of fraud amount to only a small fraction of the literature. Other problems are more important in relation to overload, for example Patrick Wilson's statement that research groups "may become conceptually inbred, insular, provincial in a bad sense."

It is the aim of research, development, science and scholarship to produce valuable knowledge and information. Theories that can reveal the ways in which process can be perfected or optimized are important. As information scientists, we are specially interested in mechanisms that involve the use of information, and we are interested in factors that influence the selection and use of information sources and systems. The theory of knowledge is concerned with the general theory of the research process. Information science is concerned with special problems such as information retrieval, selection, use, overload. In the past, there has been little contact between these areas. Information science has seen itself as technological rather than philosophical character. It is to be hoped that this will change.

As Wilson points out, an important criterion for the effectiveness of research and development - as in the stock market -is the ability to

use available information. Failure to utilize available relevant information is taken to be a prima facie sign of irrationality.

There could be more kinds of theories regarding the optimal use of information in research and development. Wilson's contribution is to show, among other things, how the phenomena of overload itself contributes to "localism."

Wicklund (1990) is concerned with similar matters. He uses concrete documentation to show how some important theories in psychology in this century have been simplified. He asks the question: Why do so many psychologists tend to use simplistic theories on behalf of more varied theories? Wicklund gives one kind of explanation. He tries to find the solution in the psychology of the specific individuals producing those theories. In fact, he is trying to develop a psychology of explanations.

In 1992, I wrote about Wicklund's book: "I regard the book as important, because it deals with a neglected issue in psychological research, or psychology as a science: The apparent decay in the theoretical level in psychology. This condition is illustrated by a number of concrete analyses of psychological theories, which in the succeeding psychological research has become substantially reduced. One such example is the almost classical theory of personality by H. A. Murray from 1938.

"In my opinion, the most essential thing about Wicklund's book is in particular the concrete documentation of the apparent decline in psychological theory. There are lots of books about the philosophy and methodology of psychology, giving direction to the science of psychology, but there are relatively few books documenting the apparent decline in theory. It seems as if psychology does not exploit the best of its own theory and knowledge from philosophy and other sciences. How can this be explained?

"Wicklund's explanation of this apparent condition is in my opinion not correct. Wicklund's explanation is different from how I see these things. Wicklund sees the documentation of the theoretical decline as something less important in his book. His main interest is to use this material to give an explanation not only about the condition of psychology, but about the psychology of explainers in general. The material which I consider having the most potential value, is for the author of the book only a minor thing.

"The reason that Wicklund's and my own analysis of the central subject of the book differs so much lies in my professional evaluation of Wicklund's explanation, which I will characterize as being individualistic: Wicklund seeks explanation of the decline in psychological theory in psychological mechanisms in the persons producing those theories.

"Certainly Wicklund, in connection with his explanation, writes about interesting and relevant psychological phenomena (such as rumors and competition) which should be a part of the pattern of explanation, but in my opinion, a broader cultural and social description is needed as a background for the understanding of these mechanisms. "In my opinion the documented examples of decline in psychological theory can in part be traced to the market for psychological books (and the market for psychologists!). In a long period after World War II, the market for psychological books (and for psychologist) was "seller's market", and it was all too easy to sell even very poorly written psychology books (and to do poor research). This phenomenon is described in an article by Jürgen Kagelmann, psychological consultant for Psychologie Verlags Union, München, in the magazine Psychologie Heute October 1988. Kagelmann's main point is that the far too easy sales possibilities in the 1970's made an overwhelming production of psychological books of a very doubtful quality. All that could be printed between two covers was thrown on the market, and the market was insatiable. This is an example of a non-individualistic explanation, which in my opinion comes closer to the truth than Wicklund's explanation, even if this is not a full explanation.

"Therefore, in my opinion, Wicklund has a tendency to individualize and psychologize a social problem, and his book contains in a way a contradiction. Wicklund acts in this book also in the role of "explainer", and he too has a tendency toward a very simplistic, positivistic theory, which the book is actually meant to fight against." (Hjørland 1992, 196-7).

One reason I have brought Wicklund's book into this discussion is that Wicklund offers concrete documentation for phenomena that Wilson only presents as theoretical possibilities. In my opinion, the phenomena treated by Wicklund could well be the result of the mechanisms that Patrick Wilson describes as reactions to overload.

This kind of negative development in knowledge is not simply hypothetical. The problem has also become visible in the public debate, raised by such books as Bloom's 1987 best-seller, *The Closing of the American Mind.*

I have demonstrated that the unhealthy influences described by Wilson seem to be related to negative tendencies that can be observed and documented in the real world. I will now try to show how the kinds of conceptual changes related to the researcher's perception and attitude toward the research process can contribute to the production of overload. As we have seen in the cases described above, there may be a kind of vicious circle at work in these trends. Overload may produce oversimplification and oversimplification may produce yet more overload. Could there exist more common, general influences that play a role in the production of both oversimplification and overload?

What is the central content of the conscious or unconscious attitudes concerning science that contributes to the large scale publication of trivial information which in turn contribute to overload? In my opinion, this is directly connected to paradigms in the contemporary philosophies of science. Johan Olaisen (1991) directly associates the words positivism and trivialism. It is also my opinion that the scientific attitudes which can best be characterized as positivism must take a very large part of the blame for the phenomenon of overload.

I shall try to substantiate this claim here even though my treatment must be brief. I hope that information scientists will study these phenomena in much greater detail in the future.

Positivism is difficult to define. It takes many forms and shapes and there are many attitudes toward it. Kolakowski (1969) suggests that positivism is defined by four essential characteristics: phenomenalism, nominalism, the neglect of values in knowledge collection, and the unity of the sciences. These four characteristics could be supplemented by others.

The first characteristic is phenomenalism. Phenomalism is the identity between phenomena, the perceived and the essential, the reality or the identity between surface and deeper reality. Problems are seen as transparent, without theoretical presuppositions. This leads to technical, mechanical, anti-hermeneutical, anti-scholarly and anti-intellectual attitudes.

The second characteristic is nominalism. Nominalism is the reduction of general concepts to verifiable concrete concepts. Nominalism gives priority to atomism rather than to holism. It involves the isolation of phenomena from their context. It concentrates on collecting facts in the form of easily measurable, concrete phenomena. The goal of

nominalism is an easily managed operationalism with its priority on precise and exhaustive descriptions at the expense of describing essential qualities or offering theoretical analysis.

The first characteristic is the neglect of values in collecting knowledge that can be summarized by attitudes that are represented as neutrality, objectivity or scientism. These suggest that values play no part in the gathering or development of scientific knowledge.

The fourth characteristic is the concept of the unity of the sciences. The unity of the sciences can be described as reductionism and is characterized by methodological individualism.

It is important to note that positivism is not identical with empirical research or with natural science. Positivism is an extreme and abbreviated view of empirical methodology. Logical positivism has never used the important methods of describing and classifying phenomena which have shown their value in other disciplines. The positivist influence is not well understood in the scientific literature and many people who deny positivism–such Karl Popper, with his denial of inductionism–can be seen as positivist if positivism is defined in terms of the characteristics cited above. The positivist movement aimed at a strong, objective science, but the central parts of its theory worked against its own purposes. Positivism was in fact open to subjectivism and in reality, it developed onto an ideology rather than a strong and explicit theory of science.

The main reason why positivism produces overload for the users of scientific literature is simply that positivism is an invalid theory. If scientists begin with invalid assumptions, it is hard to obtain true and useful results, although it is not impossible. It is therefore urgent for science to develop new explicit, sound theories of science and to communicate these theories to those whom they concern.

More concretely, positivism produces vast amounts of trivial, useless information by collecting data without the guidance of sound theory. According to Brittain (1989, pp. 99-100), "Experimentation has produced enormous amounts of data in the social sciences. Proponents of the method have been at the forefront in the analysis of the data, using statistical analysis since the 1920s, and many social scientists were just as keen as physical and biological scientists to use computers for data processing and analysis, when computers became widely available in the 1960s.

"The results of experimentation in the social sciences have been

disappointing. When assessed in terms of the development of useful rules, principles and verified theories, little has been achieved. When assessed in terms of solving social, political or psychological problems, there is little demonstrable success. At best, the relationship between the results of research and the application of research results is tenuous and often difficult to establish.

"The lack of successful experimentation in the social sciences can be attributed to a combination of factors. These include: unstable terminology; the absence of operational definitions of concepts and variables; the impossibility of identifying all the variables likely to influence the outcomes of experimentation; problems in the control of variables that can be identified; the absence of good theory that can generate testable hypotheses; fudging the verification process of moving from data and observation to the substantiation of hypotheses, and in turn, the verification of theories.

"The simple conclusion to be drawn is that data is not knowledge. However, those who support experimentation and controlled observation are sometimes difficult to persuade otherwise. The libraries of the world are full of documents containing unprocessed and unusable data. The data bank movement in the social sciences has perpetuated the mistaken belief that mountains of data are worthwhile and that if enough data are collected, analyzed and stored, benefits will result and the social sciences will progress. This belief is mistaken: it is characteristic of alchemists or mystics rather than scientists or aspiring scientists."

Brittain continues, adding that "the first experiments in the social sciences took place in psychology laboratories in Germany in the 1870s. Over 100 years of social science experimentation has failed to produce a set of theories, principles and agreement. If the criteria used to evaluate scientific experimentation are applied to the social sciences, the latter must be judged to have failed."

Loftus (1991) continues on this theme, writing that, "since the 1940s, the practice of hypothesis testing has been seeping into all nooks and crannies of social science methodology. Today, hypothesis testing constitutes the major foundation of data analysis in experimental psychology; it is used to justify conclusions from data in over 90% of the articles in major psychological journals.

It isn't actually the validity of these assumptions that I worry about. It's well known that most relevant sampling distributions are robust

against most assumption violations (and, of course, there are always non-parametric tests). Rather, the problem is that our early (and continuing) imprinting on these assumptions and traditions engenders strong biases against formulating theories incorporating other, perhaps more interesting and realistic, assumptions. Thus, psychological theory becomes generic analysis-of-variance theory and the potential for insight is lost."

The "publish or perish" syndrome that originated in America and has now spread worldwide can also be connected to positivism. The tendency to evaluate researchers by quantity, to measure the number of pages, the number of citations and so on rather than considering content or quality has clear positivist tendencies. This tendency in evaluation has had a profound and unfortunate influence on scientific production. It favors the production of easily produced documents at the expense of more difficult, long-term investment. It also favors certain kinds of collaboration such as multiple authorship and positive recognition of theories with the primary goal being higher scores in this positivistic form of evaluation.

Patrick Wilson asks the question, "Is science rational?" One way to find out is to analyze the explicit and implicit assumptions governing science. In my opinion, deeper knowledge about positivism and other theories of science makes it possible to make such analyses. I also think that I know the answer: A science can only become rational if it is guided by a rational theory of the scientific process. The theory of science–for example, the understanding of positivism and its influence–is in a weak condition. Much science today is guided not by a rational theory of science, but to a great extent by ideologies. In my opinion, science should support values such as maturity, scholarship, responsibility and integration rather than predominant positivistic notions. This would have many benefits including the reduction of overload. It should be remembered, however, that the positivist approach was once a reaction to the unsatisfactory condition of metaphysical dominance. Positivism had great value but the positivist tradition grew beyond its role as a balancing corrective to become a dominant ideology. We should not go to the opposite extreme.

In a recent book review (2), Gergen (1993) considers the situation today, which he calls the post-empiricist phase. According to Gergen, research practices in psychology have remained virtually unchanged for 50 years although the philosophical arguments that once lent rea-

son for optimism to these practices have long been moribund. Gergen regards himself as a social constructivist and he sees the book he reviews as a heroic but rather hopeless defense for scientific realism. This says much about the situation today. It is necessary to bring research practice into alignment with modern philosophy of science and it is extremely important to develop a further clarification of these positions.

To bring this section to a conclusion, we can summarize by noting that we have seen how some kinds of conceptual changes in relation to the perception of science and attitudes toward science and the scientific process produce information overload. In order to reduce information overload, the theory of science must be strengthened in research, in educational programs and in information science. For the information science professions, it is important to be able to identify different theoretical paradigms–and even vague cues for them–in the literature. The paradigms are not explicit or visible. They are complex and they can not therefore be identified by simple mechanical tools such as automatic indexing techniques, neural networks or filtering techniques. In spite of this, paradigms represent one of the most important structures in the subject literature. The perception of these structures is relevant–and important–to almost all the activities of information professionals, including information selection, subject representation and information retrieval.

Short-sighted Pragmatism

I have a deep respect for the American pragmatist philosophers such as C. S. Peirce, William James and John Dewey. The criticism I raise here is not aimed their work or at least not at a great part of it. William James warned against the extreme positivistic attitudes that came to dominate American psychology after 1913 with the birth of Watson's behaviorism. Its influence persists to the mechanical cognitivism of our time, though some small indications suggest that at least psychology and cognitive science are developing in a less positivistic direction (Gergen, 1993).

The word "pragmatism" is ambiguous. On one hand, pragmatism is a valuable philosophy which look at the human mind, human language, human knowledge and the sciences as developments in the course of biological, cultural, social and individual evolution. Life and

biological action precede knowledge. Knowledge is therefore constructed in such a way that an application of well constructed knowledge will directly or indirectly serve life and action as an internal determinant in organisms. This implies that it is only meaningful to study the mind, language and knowledge as instruments for the management of life. In other words: knowledge can not be studied in isolation or inside the mind through methodological solipsism or individualism. It must be studied as a developmental process which involves looking at the biological, cultural and social environment to which knowledge is an adaptation for organisms.

In my view, the cultural-historical psychology that originated in Russia–today called activity theory or sometimes, in German, critical psychology–is the best developed representation of this view. Key authors such as L. S. Vygotsky and A. R. Luria are well known in English scientific literature and recognized as classics. Many others, such as A. N. Leontjev, have yet to be discovered despite the fact that they are part of the same school of thought. In my opinion, this tradition has something important to contribute to contemporary positivistic, mechanical cognitive science and could be characterized as pragmatism or as realism or materialism in the best sense of these words. The pragmatic philosophy of science does raise important problems. True knowledge is not absolute but is generally the most suitable means to human adaptation. How is one to determine when the practical solution to a problem of real life is also the sufficient proof of a theory? In the days of the Soviet Union, for example, many Marxists saw the ostensible success of this communist state as proof of Marxist theory. The subsequent collapse of the Soviet Union also meant the collapse of this form of Marxist theory and the impossibility of proving theories by citing the concrete achievements of the Soviet state. This example shows a key problem of pragmatism. Even so, it does not refute the value of the pragmatist principle of viewing knowledge an adaptation to specific environmental problems which must be described as part of the theory of knowledge.

We must therefore distinguish between two kinds of pragmatism.

The first is a long range theory that views the development of knowledge as an adaptation to the environmental problems of living organisms.

The second is a short sighted pragmatism which only looks for arguments about what is purposeful in the single situation, for the single

person, for single groups of persons or for the single organization or enterprise.

In this section, I shall try to illuminate, how this short term pragmatism has become an increasingly important factor in information problems, thus a key factor in problems related to the phenomenon of information overload.

To begin, let us consider arguments in information science by Lancaster (1991, 10). Lancaster identifies himself with a pragmatic approach to indexing. He finds that much philosophy in information science about "aboutness," "subject" and so on "have failed to clarify the situation, at least as far as the task of subject indexing is concerned." He also cites Patrick Wilson (1968) where Wilson says that subject indexing faces "intractable problems." Lancaster asks whether it is necessary for us to understand "aboutness" in order to make good indexes? Isn't it enough to realize that a given document is of interest for a given group of users while it contributes to our understanding of the topics x, y and z?

Lancaster's view reflects some of the ambiguities of pragmatism. On one hand, he offers the rudiments of a pragmatic theory of the long-range kind. He suggests that "aboutness," "subject" and so on must be understood in the context of the function of indexing for users. On the other hand, he also uses pragmatism in the short term perspective, in that he does not find it necessary to develop a deeper and more precise theory about indexing, "subject analysis," "aboutness" and so on. But concrete theories, conceptual clarifications and the like can only be evaluated from a theoretical basis. You can not avoid theoretical problems by claiming to be pragmatic. You have to show which concrete changes in theory and definition are required. You must understand the precise way in which existing theories fail.

I agree with Lancaster that a librarian in a specific user context–for example, a rubber-factory–must index documents according to the user's specific requirements. But this is not all there is to say about interpreting the subjects of documents. Biological phenomena can be classified according to scientific principles in groups established by botanical and zoological taxonomies. They can also be classified by narrow pragmatic concepts such as useful animals, pets, vermin and so on. In ordinary speech, only the latter system of classification counts as pragmatic. But scientific taxonomies are pragmatic in a deeper way. They are abstract, generalized categories, which reflect

an efficient and thereby pragmatic way to organize human knowledge on a deeper level. In my opinion, the scientific organization of knowledge must in general be seen as more important, even though narrow pragmatic organization is appropriate under special circumstances. The difference between the two ways of organizing knowledge reflects the difference between applied research and basic research.

In information science, there has been too little interest in long-range pragmatism and too much in short term pragmatism. It is important to have a deeper understanding, for example, of subject analysis. This kind of understanding is necessary in order to have a background against which to measure, for example, the efficiency of different subject-access points such as descriptors and citations. According to Saracevic (1992, p. 6) information science has been dominated since the days of Vannevar Bush by an ideology of technological fixes based on the attitude that complex informational problems such as the information explosion can be solved by simple, technical solutions. Vannevar Bush was in tune with the spirit of his times and his approach was strategically attractive. The disadvantages of this view are much the same as other forms of short term pragmatism and of positivism in blocking a deeper theoretical analysis of problems and their interrelatedness. Information science thereby developed a distinct tendency for intellectual discontinuity and a way of thinking by means of quick solutions that tend to spread in an uncontrolable way.

It is important to understand the limitations of this kind of view. The absence of interest in basic theory–long term pragmatism–has brought information science into a critical situation. There are many short term contributions and an overwhelming volume of fragmented and disintegrated literature. There is little that can be characterized as substantial principles to support learning for students and professionals in the area.

Both F. W. Lancaster and Patrick Wilson are among the best contributors to information science, but I do not share Lancaster's view of Wilson's analysis of the concept of "subject." Wilson's analysis has also been valuable for my own work on that concept (Hjørland, 1992), a scholarly analysis that represents a marked intellectual approach contrary to the tendencies of short term pragmatism. One can disagree with Wilson's concept on some concrete points but his theoretical analysis is an important step towards insight.

These tendencies in information science are only a special example of modern tendencies in research. The trend is to move from the deeper, general and long term to the concrete and short term. The reasons for this, we are told, is that research must be relevant. The issue remains that this may be a mistake in judgment. The question is whether the pressures for supposedly relevant results make research superficial, useless and irrelevant by failing to frame the right problems and understand the genuine foundations of those problems. The question is whether short term pragmatism is indeed useful or pragmatic at all. One reason for the strong influence of behaviorism in American universities was its strong support from industry. But has American industry been well served by behaviorism? Behaviorism is a primitive and infertile theory that fails to catch the essence of the human psyche. It must therefore be a bad guide to management theory and services for industry. What really is useful and pragmatic can only been understood through a deeper understanding of the object of research. This requires other principles than positivism and short term pragmatism. It requires scholarship.

The tendency toward short term pragmatism corresponds with the conception of "satisficing" cited by Wilson (1993, 5) "to say that people satisfice in the use of information simply means that they 'use search routines that are limited in extent, narrow in conception, and suboptimal in outcome'." My aim here is to demonstrate that tendencies such as satisficing are not simply individual, psychological phenomena, but social and cultural trends. I find many signs that science in our century–especially the social sciences–have moved increasingly toward short term pragmatism and away from theoretical systems that integrate the knowledge of the past, along with the contributions of philosophy and other disciplines.

What is needed in all kinds of research, including research in information science and information work, is a broader problem perception, a more generalized perspective that explores problems from a deeper acquaintance with scientific and philosophical theories in their historical and cultural context. We must cease the overproduction of unrelated facts. The task of scientists should not be to follow certain peer-defined, satisficing norms of research defined by quantity, method or content, but to analyze the scientific production in relation to its real contribution to society. For information seeking, this means that information retrieval should do more than deliver relevant responses

to specific queries. To some extent, it should also do what the old fashioned classification systems can do by giving the researcher a map of the landscape of research. It should offer the researcher some kind of perspective on the structure of science and his place in it. Its task should be to provide consciousness rather than merely delivering information. The more that research is integrated into well organized, general structures, the less the risk of overload.

The Meaning and Consequences of Information Overload

In the first part of this article, we saw that basic conceptual patterns, basic ideologies, misguided concepts, outdated theories of science and short term pragmatic attitudes produce information overload. Perhaps our analysis has shown that information overload is a surface problem that results from problems of ideology, lack of quality, positivism, pragmatism and the philosophy of knowledge and of science. It may be a *symptom* of deeper problems rather than a well defined problem in its own right. If knowledge is seen as something which facilitates living and action, then the overproduction of knowledge is a contradiction in terms. You can have problems with overproduction of low-quality articles and problems in knowledge production system such as bad quality, fragmentation, the failure to distinguish between trivial and important knowledge and so on. But this is not overproduction or overload of knowledge or information. Rather, it is the overproduction of specific kinds of data and inadequacies in the knowledge producing system as well as in the knowledge dissemination and knowledge use systems.

Overload is not just connected with conceptual patterns and attitudes in the knowledge producing and consuming sectors. Its is not only related to ideas. It is also related to material factors in social cooperation and the organization of knowledge production and dissemination.

Information retrieval and information seeking depend to a large degree on cooperation between knowledge producers, knowledge intermediaries and knowledge users. The principles of information retrieval and information seeking can not be understood on an individual basis. One must consider the sociology of knowledge producers and users.

Wilson (1993) presents important arguments for studying specialties rather than individuals in information systems design. Page 379: "We have put the communication problem as one of communication among specialties rather than among individuals. This approach is meant to reflect the fact that the main way in which information from outside affects a specialty is by being recognized by the group as being impersonally, "objectively" relevant: as providing a point, supporting a hypothesis, making a theory less likely, demonstrating an effect, and so on (cf. Swanson 1986). It is not how some individual is affected but how the specialty as a whole is affected that is in question: it is the group as a whole that has to be persuaded that the information has an appropriate logical or evidential status. ."

He continues on page 380, where he writes that "it is not obvious that we should be much concerned about efficiency on an individual basis; rather, it is collective efficiency that should be our concern. Scientific knowledge is a collective product, and as such the product might efficiently exploit available information even though any given individual was hopelessly inefficient and even though each individual was sometimes inefficient."

I could add arguments of my own. In Hjørland (1993) and in Hjørland and Albrechtsen (1995) I have discussed this matter in depth and I argue that information science should regard specialties, disciplines, branches as the focus and unit of study rather than individual user behavior as is predominant today. The individual's behavior should be regarded in light of his place in the division of labor. If the contribution to knowledge of a single scientific document approximates zero, as Wilson states, this means that the opinions of any individual scientist regarding the information needs of science are problematic.

When the individual researcher is overloaded, this can be seen as a function of conditions in the scientific discipline in which he works. There is an extreme difference between mature and immature disciplines. In mature disciplines, the job of collecting and evaluating information is divided among members of the discipline in a relatively clear structure. In addition, there may exist many definite criteria defining relevant information and irrelevant information. This lowers the degree of overload on the individual. In immature or unhealthy disciplines, the criteria of relevance are unclear and the division of labor is blurred. Fields influenced more by short term or pragmatic tendencies

than by problems arising from the object of study do not integrate knowledge into a clear structure but tend to rediscover the same things in many different environments, under different names and concepts and so on. Such knowledge tends to be scattered and scattered knowledge contributes to information overload. In extremely unhealthy disciplines, true knowledge production is nearly an illusion. What counts in these fields is not genuinely new knowledge but the use of formal methods, the forms and quantity of publication and artificial criteria set by influential researchers or their employers. Things must look scientific. Genuine, critical researchers who attempt to solve real problems rather than meeting a set of peer-established norms face difficult conditions. Immature disciplines are often promoted in ways that should only be seen in mass media advertising.

Today, there is an increasing interest in evaluation studies of science, research and learning (Christiansen and Christiansen, 1989; Chubin and Hackett, 1990 and Evered and Harnett, 1989). Again, some of this research on research is mature, but much of it may not be since it only amounts to statistical indicators of quantity and influence, even though this is often misinterpreted as an indication of quality. Mature evaluation studies are not simply empirical studies. They also integrate theoretical knowledge about the philosophy and sociology of science. Information science must be closely related to such studies of evaluating knowledge. This is the only way we can do our job. We must understand the user's situation, his problems of coping with overload, his problems with identifying relevant or non-trivial information and so on.

The phenomenon of overload tends to be self-perpetuating. The Norwegian psychologist Karl Halvor Teigen writes:

"Elementary statistics indicate that with a greater number of publications, the probability that researcher A and researcher B by chance do have knowledge about the same article is decreasing. Therefore, we can to a lesser and lesser degree count on the assumption that we and our colleges share the same background or experience. Even if perhaps each of us does know more than before, it becomes more difficult to have rapport with and establish a common platform with our peers. More time will be used to orient each other about our different points of departure, less time will be spend on going further together.

These and corresponding circumstances will teach us to accept

that there exist more between the roof and floor of the library than our teachers in school dared to dream about. This is a lesson in tolerance if not a lesson in resignation and apathy. As tourists in Babel we must accustom ourselves to the idea that most terms in the subject literature are not meant for our ears. Even professionally absurd postulates are therefore, to a large extent, left in peace. Only the researcher who is by nature unusually militant or self-tormenting will voluntary arm himself to a battle against these stumbling-blocks when there exist hundreds of ways to get around them.

With a greater supply of literature than a demand for it, nobody can claim to have given his opinion once and for all and leave the future to be the judge. That something is printed is no guarantee that it will ever be read. After some years, according to majority opinion, it will simply not be worth reading. In times with overproduction, the glitter quickly will wear off research. The person who made a discovery fifteen years ago or promoted a point of view which in his opinion still deserves attention can not obtain attention in other ways than by mentioning it again. In addition to selective and unreliable long term memory, the reading community seems to be equipped with a short term memory which functions according to the principle of reverberating circuit or rehearsal. Theories and discoveries which are not kept warm by continuing references and quotations quickly die. If the past is to continue to play a part in current affairs, it must be continually rediscovered and brought back to light. In this way, the past contributes its own stream to the overflow of literature that threatens to drown research.

The principle of publish or perish is no less relevant for the single researcher who wishes to be heard. It is difficult for one single person to receive attention among ten thousand. The chances of being noticed - not necessarily perceived or understood - increase by a factor of five for the person, who is creative enough to reiterate what he has to say in five different ways. Because this is a recognized and widespread principle, the ten thousand will soon sound like fifty thousand and the chances of reaching the individual reader will be the same as before. Our importunate and provident friend will therefore initiate a cooperation with a range of co-authors in order to increase the visibility by a factor of five for the second time. His colleagues will not be slow to follow his example.

Again, we are dealing with a mechanism which contributes further to the acceleration of an activity in publication which has already be-

come boundless. We can establish that the forces of a market which no single individual can control are at work here. This implies that the single researcher must use an increasing amount of his time simply to keep oriented and he must use increasingly more time to orient others. Gradually, as the sum of these two quantities approach and exceeds the total time available, you can ask what more there is to be informed about." Teigen (1985, 69-70) (Translation from Norwegian by BH, edited in English by Ken Fredman)

The vision that Teigen portrays looks dark. The consequences of the overproduction of scientific literature will be a literature that increasingly loses its integrity and serves as a well nurtured collection of controlled data and carefully argued theories to an even smaller degree. Instead, it becomes more and more like the mass media where serious argument, scientific methods and the theories of science do not count as much as commercial considerations.

William D. Schaefer is a professor of English and former editor of one of the leading journals published by Modern Language Association. Like Teigen, he has developed a pessimistic attitude toward the quality of much scholarship. In his mind, one of the reasons for this state of affairs is the overproduction of totally unqualified doctors. He tries to describe a solution with a sense of humor:

"We received more than 1,200 submissions, around 1,000 of which met the basic criteria ... From the 1,000 articles submitted over a two year period we found only 53 good enough and important enough to publish ... perhaps 30 that were truly worth bringing to the attention of the entire profession.

What if, along with any article submitted to a scholarly journal, an author had to include a signed statement agreeing that the editor had the author's permission to send a copy of that article to a special MLA journal called Twaddle, a quarterly collection of the 10 worst articles submitted to scholarly journals during the previous three months?

Instead of a William Riley Parker Prize for the outstanding article in PLMA, we might have a William David Schaefer Prize for the dumbest single article to appear each year in Twaddle, with free offprints to the author's department chair and dean. I even dreamed of the day when the following conversation could occur between a chair and a non-tenured member of the department:

Chair: Smith. I've had reports that you've been attempting to publish again.

Smith: It's true; it's true. I'm sorry.

Chair: Even worse, there's a rumor going around that you're the John Smith who published this asinine analysis of "My Last Duchess" reprinted in Twaddle.

Smith: I did it; I did it.

Chair: How long did it take you to write that piece of junk?

Smith: Six month, on and off.

Chair: Six months that you could have devoted to something worthwhile, like reading or preparing classes or talking to students. Beware, Smith. If this happens again you haven't a prayer of getting tenure in this department!" (Schaefer, 1990).

Today there is an intense debate about quality and quality management in all sectors of society. This wave is now hitting research, higher education, libraries and information centers (Wormell, 1990).

Some commentators feel that much of the debate on quality in the academic sector is not always serious, but rather a manifestation of ideological tendencies, an attitude that advertises science as an amusement park (Brüsch 1991; Øhrgaard 1992).

Other commentators and analysts find that part of the academic sector has been subject to serious intellectual decay (Bloom, 1987; Jacoby, 1977; Wagner, 1977 and Schaefer, 1990). These authors do not regard the debate on quality as an ideological trend, but as a reaction to serious problems in the academic sector itself.

Both positions may have some degree of truth. There are serious problems in quality and quality-management but the initiatives taken to solve those problems are often not serious enough. Many seem to consist of superficial concepts from the literature of management at the expense of deeper studies of philosophical or sociological analyses.

The most serious threat we face is the lost of confidence the academic sector. To many, this sector is seen as largely irrelevant to the economics of the grater society. If this view triumphs, it would be a victory for irrationality, setting society back to the conditions that existed before the Enlightenment.

Much is at stake. In order to regain public confidence, the academic sector must itself be rational and exercise the highest degree of scientific and scholarly character in its self-analysis and quality control. There is conceptual muddle in the theory of science. Its is difficult today to find qualified articles, encyclopedias or handbooks that illuminate basic concepts such as objectivity in light of the crisis in positivistic views. This is but an example of much important work that has to be done.

I'll finish this section with the words of Horrobin's (1990) The Philosophical Basis of Peer Review and the Suppression of Innovation:

"I cannot put my conclusions too strongly ... it is the duty of editors to encourage innovation as well as to ensure quality control ... many scientist-reviewers are against innovation unless it is their innovation ... The idea that all scientists are peers simply will not do ... Find a reviewer with generosity of spirit who will not recommend rejection because of those faults that can be found in any article but that do not challenge the fundamentals of what is being said ... Never forget the possibility that even the most eminent and urbane of reviewers may occasionally be corrupt or malign or that lesser folk may be acting under duress ... Fifth, be an editor ..."

The Role of Information Specialists in Relation to Overload

What does this analysis mean for information professionals? One implication is that it is important for librarians and information scientists to be serious interested in the quality criteria of documents as well as in their own services and in the efficiency of the institutions that evaluate document and their authors. When librarians select documents, they face problems similar to those of scientific editors. Serebnick (1991) offers a valuable start in "identifying unethical practices in journal publishing." Moran and Mallory (1991) are working with the same kind of problems and they question the institution of peer review in terms of its value to academic freedom. They argue that librarians should actively look for and oppose tendencies to oppress intellectual freedom and they urge librarians to support whistle-blowers. In order to evaluate tools used in selection, studies on the validity of quality judgments made by central selection tools such as *Choice*–for example, Leavy's 1992 research – are important.

Many kinds of evaluation research are based on bibliographical data. This implies knowledge of databases and other issues in which library and information specialists are trained. Rice and Stankus (1983) deal with the ethical responsibility when they are asked to give data which are to be used as indicators of quality:

"Citation data is frequently used to evaluate the publications of members of academic faculties for tenure and promotion decisions. Discusses the impact of online availability of citation data and provides guidance in the interpretation of search results by librarians. Concludes that academic librarians who are asked to supply information for tenure or promotion decisions have an ethical responsibility not to supply the requested information but to educate the requester in the limitations of any search techniques used (manual or online) and the types of factual information that can be supplied. Librarians should require from requesters that they should participate in the search process (especially in the case of online searches) and provide a clear statement of what is being asked for."

There exists little literature about how to identify the validity of an information source in literature and information seeking or information retrieval. Shirato (1991) represents an attempt to teach this. This knowledge must be based on a high level of understanding of methodology and theory of science and knowledge.

Consequences for Information Retrieval Theory

In the concrete information retrieval situation, the searcher can use several strategies to cope with large retrieval sets. This is the most concrete problem of overload felt by the searcher. There are no absolute indicators giving the best strategy for limiting large sets. Even so, since the searcher often confronts large sets, research on this issue is mandatory even if it can only point to rules of thumb on search strategy. There exists a limited literature on this problem, such as Bjørner (1991) "Evaluation output by quick title scanning - on-screen or off-screen?"; Hickeey and Prabha (1990) "On-line public catalogs and large retrievals: Methods for organizing, reducing, and displaying"; Kinnucan (1992): "The size of retrieval sets" and my own work (Hjørland, 1985) "The selection strategies of researchers in conditions of 'overload'." (In Danish).

For example, when you read a newspaper, you are not only selecting articles by topic or theme. First and foremost, you select the newspaper that you prefer, that is, you select a group of journalists and editors, whose qualifications, points of view and standards you respect. This tendency also affects the selection of scientific journals, though to a lesser degree. In medicine, for example, there exist a

hierarchy of esteem among journals. On the basis of core lists (Usdin, 1979) selective sets of journals could be built as saved programs to be executed during a search. Technically, it would be easy to make to limit large sets according to specified sets of journals in information searches. This would mean a less arbitrary way of limiting large sets than by year of publication. It would be easy for librarians and information specialists to study journal preferences and to help implement such possibilities since sets of journal preference could also be individually defined. You could imagine the construction of one or more critically annotated databases on journals, and the technical possibilities of selecting journals from evaluated files to use the result as input for another search method that limits large retrievals. In that case, it would be important to have interfaces like Windows and simultaneous searching of different databases in different windows. This idea of quality filtering of databases is not totally new, as shown by Moore's 1989 article on the problems of dealing with the ever-increasing mass of biomedical information that he terms "journalistic blastoma." *Integrated Academic Management Information Systems* has suggested the use of quality filters that sift the good from the bad. Moore finds that the complexity of the task is staggering. Quality is subjective, and Moore's approach is to teach users the skills needed to evaluate the literature. In my view the most important thing is to realize that:

1. There exist different norms of quality. It is important to develop systems which provides the user with alternatives based on different criteria of quality.

2. It is important that retrieval systems do not cope with quality in a positivistic way as something objective and directly measurable. They must utilize the important value-added work of editors and publishers as wells as reviewers, critics and others. They must make this kind of information easily available in the retrieval situation.

3. Views of quality should partly be tied to scientific theories and paradigms. Criteria for quality, relevance, essentiality and so on should be linked to collective tendencies along with individual, subjective perceptions. It is the aim of science, scholarship and learning to develop criteria for quality and relevance. This is implicit in the scientific methodology. Information systems should be part of this collective process. On one hand, information systems should utilize criteria and evaluations already established by others. On the other, it is the

aim of information retrieval to serve science. Therefore information systems should support the development of quality criteria of outside the system itself.

It is also important for information scientists to regard the individual searcher's real and hypothetical set of alternative ways to limit large retrieval in a holistic way. Some of these possibilities are:

- expand the use of the Boolean "and"
- expand the use of the Boolean "not"
- limit the use of synonyms and Boolean "or"
- use specific methods (such as longitudinal studies)
- select by esteemed journals
- select by review articles and book reviews
- select known authors
- select by field (descriptor, major descriptor, title and so on)
- select most cited authors or documents
- select by availability
- select by theoretical criteria such as phenomenology, Marxism, systems theory and so on
- select by disciplinary criteria
- select by novelty
- select by format, for example select by books but not journals
- select by language and/or country
- create other selection criteria

Some of these search tactics are well known in information science. For example, it is well known that it is difficult to use the Boolean "not" and beginners often use this command in an inappropriate way.

Other tactics are not discussed in the literature of information science. Klein (1990) offers a valuable exposition of the problems of the interdisciplinary borrowing of concepts and theories. This is but one example of problems in the theory of knowledge to which we should pay greater attention and which confirms Wilson's and my own thesis about the specialty as the unit of analysis.

On the whole, the collected set of possibilities of coping with overload in the retrieval situation has been much neglected by information science. It is important not only to have a clear overview of the different possibilities. It is also important to know the strengths and weaknesses of each tactic, some of which may differ from one domain of knowledge to another. Often users apply tactics such as using the

Boolean "not" or novelty, limiting a search by the year of publication. If information scientists claim to be experts in information retrieval, they should be able to warn users against inappropriate tactics. This presupposes reasonable arguments for the appropriateness of each tactic. Arguing the inadequacy a search limited to new literature requires knowledge about the structure of the literature and the fallacies of theories of obsolescence. Experienced searchers often develop feelings and implicit understandings. These feelings must be made explicit, tested and developed into professional knowledge that can be taught. The concept of paradigms is valuable in this process. It should be supplemented with concrete knowledge about paradigms in different subject areas, and different ways in which the cues of a document's paradigm can be identified.

Much research in information retrieval seems naive and unrealistic in the light of the problems of information overload. Many approaches tend to recall trivialities and to strengthen given ideologies rather than allow the user to differentiate and select. This applies to both the statistical or probabilistic approaches and to approaches based on user modeling and expert intermediary systems. Some researchers see hope for the technique of neural networks. Only two existing approaches seem to be able improve the situation: The first is citation indexing. This gives the user additional cues in identifying and avoiding trivialities by using the evaluations of reviewers and others. The other approach is hypertext. This technique allows users to build systems in which documents are linked to comments, reviews, evaluations and cues found in other documents.

Our main conclusion is that the atomistic approach in most modern information retrieval research must be supplemented by a holistic approach which considers the structure of disciplines and domains of knowledge. We must inform the user not only about documents relevant to a request but also about the structure of the domain he is navigating.

The modern era of information retrieval began in about 1950 when Calvin Mooers introduced the term information retrieval. Experiments of the Cranfield type started at about the same time. Until then, classification research was occupied by philosophical problems, by the concept of disciplines and by structures of knowledge. These approaches were neglected by the extremely empirical and atomized information retrieval tradition. They deserve renewed interest.

The reason for my strong support of Wilson's (1995) thesis is that the filtering systems now seen as the important goal of information technology can only be designed based on the real knowledge of user needs.

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Notes

(1) *Review of Gerd Gigerenzer et al.: The Empire of Change: How Probability Changed Science and Everyday Life. Cambridge, England, 1989.*

(2) *Review of John D. Greenwood: Relations and Representations: An Introduction to the Philosophy of Social Psychological Science. New York: Routledge, Chapman and Hall, 1991.*